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Biogeosciences Discussions

Interactive comment on "CO_2 air-sea exchange due to calcium carbonate and organic matter storage: pre-industrial and Last Glacial Maximum estimates" *by* A. Lerman and F. T. Mackenzie

Anonymous Referee #1

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A. Lerman and F. T. Mackenzie compute with a 2 box ocean model the air-sea CO2 fluxes owing to CaCO3 precipitation and organic carbon production/degradation during the Last Glacial Maximum (LGM) and during pre-industrial time.

As already addressed by the other 2 referees, the manuscript is long and at times difficult to read. I strongly suggest that section 2 be shorten to essential equations and transferred to section 7 (Appendix). A careful editing is required in text and referencing.

My main concerns are:

1 - Pages 433 and 446, comparison of the "psy" factor described by Frankignoulle et al. (1994) with the "theta" factor introduced by A. Lerman and F. T. Mackenzie.

The "psy" factor is the molar ratio of CO2 released to the surrounding waters (and not the CO2 exchanged between the water and the atmosphere as assumed by A. Lerman

and F. T. Mackenzie) to CaCO3 precipitation (Fankignoulle et al. 1994). The potential air-water CO2 exchange is further controlled by net ecosystem production (Gattuso et al. 1995). The actual air-water CO2 exchange will be also controlled by the water residence time (and volume of the water body) within the ecosystem and the air-water CO2 gradient of incoming seawater (Gattuso et al. 1996; Frankignoulle 1996; Bates 2002).

The “theta” factor is the ratio of CO2 released from various processes (“riverine DIC inputs”, “organic carbon respiration”, “primary production” and “CaCO3 precipitation”) to CaCO3 precipitation.

Thus, the 2 factors are conceptually different, not comparable and the attempts of comparison should be removed.

Furthermore, the “psy” factor applies to an ecosystem level (e.g. a coral reef as originally developed by Fankignoulle et al. 1994) while the “theta” factor applies to a global oceanic scale. Which brings us to the actual usefulness of such a factor, since there is a strong temporal and spatial (both vertically and horizontally) decoupling of the processes grouped in the “theta” factor. For instance, in the open ocean, primary production (in surface waters) is usually decoupled from respiration (sub-surface to intermediate waters). Degradation of river organic carbon occurs in near-shore waters (estuaries) while CaCO3 production mostly occurs in coral reefs that usually flourish in areas devoid of river inputs. The corollary of the usefulness of the “theta” factor is: what can we conclude from a two box ocean model applied to the open oceanic and coastal waters?

2 - I’m surprised how the A. Lerman and F. T. Mackenzie computed DIC and TA (and the “theta” factor). They used a constant pH of 8.35 for the LGM based on Sanyal et al. (1995), 3 different temperatures and assumed a constant pCO2 of 185 ppm to compute TA and DIC.

Firstly, a constant pH of 8.35 for the LGM is an average value that should be associated

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to a given average water temperature. From these averages and a pCO2 of 185 ppm, DIC and TA should have been computed. Then from these LGM averages of DIC and TA the various parameters (pH and “theta” factor) should have been computed from the different temperatures (5, 15 and 25°C), since DIC and TA are independent from temperature unlike the other parameters (pH, “theta” and CaCO3 saturation state). The approach used by the authors of a constant pH for 3 different temperatures is not only unrealistic but its usefulness difficult to apprehend.

Secondly, A. Lerman and F. T. Mackenzie state in page 440 that “In pre-industrial time, the ocean was a source of atmospheric CO2 owing to heterotrophic respiration of organic matter brought by rivers from land and produced in situ, as well as owing to biological calcification in the coastal zone”. If so, pCO2 in seawater must have been higher than atmospheric pCO2 although the authors use the value of 185 ppm in their computations (equilibrium with the atmosphere). Also, although the whole oceanic realm (coastal waters + open ocean ; surface + deepest layers) was probably heterotrophic in pre-industrial time (assuming a steady-state imbalance between river organic inputs and final burial in sediments), there must have been a spatial heterogeneity in ecosystem metabolism: surface layers versus deeper layers ; proximal versus distal coastal areas (e.g. Rabouille et al. 2001). This brings us again to point 1 regarding the usefulness of a two box ocean model to tackle highly heterogeneous biological and chemical processes and extremely complex feedbacks between biology and climate.

In conclusion, I agree with the other referees that this manuscript requires major modifications before being acceptable for publication in Biogeosciences.

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Sanyal, A., Hemming, G., Hansen, G, and Broecker, W. (1995) Evidence for a higher pH in the glacial ocean from boron isotopes in foraminifera, Nature 373: 234-237.

Interactive comment on Biogeosciences Discussions, 1, 429, 2004.